Loans, Liquidity, and Schooling Decisions

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Abstract

During the 1999-2000 school year, students borrowed $36 billion through the federal loan program, double the volume in 1992-93. Despite the large size and rapid growth of the student loan market, it has been the subject of little economic analysis. Does the availability of government loans affect schooling decisions? Identifying the effect of loans is empirically challenging, because eligibility for federal loans is correlated with observed and unobserved determinants of schooling. I exploit variation in loan eligibility induced by the Higher Education Amendments of 1992, which removed home equity from the set of assets that are taxed by the federal financial aid formula. I find weak evidence that loan eligibility has a positive effect on college attendance and shifts students toward four-year private colleges.

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I. Introduction

Student loans are a fast-growing market. During the 1999-2000 school year, students borrowed $36 billion through the federal loan program, double the volume in 1992-93.[1] The number of students in college did not change appreciably during this time; rather, the average loan and the proportion of students taking loans both rose sharply.[2] By comparison, grants to college students have grown quite slowly. As a result, there has been a marked shift in the composition of aid from grants to loans over the last decade.

From an economist's perspective, this would appear to be an efficient reallocation of government resources. A key economic rationale for government intervention in the higher education market is the loosening of credit constraints, which are imposed by the reluctance of private markets to make unsecured loans against human capital. While the obvious solution to such constraints is an infusion of liquidity, the traditional government response has instead been price subsidies. For example, tuition prices are kept artificially low at public colleges, while low-income students are further subsidized with the Pell Grant, a portable voucher.

There is firm evidence that price subsidies, at least in the forms of grants and low tuition, increase college attendance.[3] By contrast, we know little about how loans affect schooling decisions. Does the availability of student loans affect who goes to college, and where they

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[1] Figures are from College Board (2000). Values are inflated by the CPI-U with academic year 1999-2000 as the base year.

[2]Between the academic years 1992-93 and 1995-96, the average loan rose from $3,300 to $4,100 and the share of undergraduates borrowing rose from 20 to 26 percent. US Department of Education (1998). The student population has remained stable during the last decade, at about 14.5 million. See Table 173 in U.S. Department of Education (2000a).

[3] See Kane (1994, 1995) and Dynarski (2000, 2001). Dynarski (2002) provides a review of this evidence.

choose to go? The cross-sectional correlation between loan eligibility and schooling decisions is not instructive in this regard, since loan eligibility is determined by (frequently unobservable) characteristics that likely have their own effect on schooling decisions.

This paper identifies the effect of loan access using shifts in loan policy in the early 1990s.[4] The Higher Education Amendments of 1992 (HEA92) removed home equity from the assets "taxed" by the federal aid formula. Previous to HEA92, each dollar of home equity reduced by three to six cents the federal aid eligibility of families on the margin of receiving more aid. Since home equity comprises the majority of wealth for most families of college-age children, this rule change swept many families into aid eligibility.

Those with the highest home values saw the greatest boost to their aid eligibility.[5] In some specification, I therefore use home values as a proxy for exposure to the rule change. Specifically, I use state-year variation to estimate a time-varying relationship between home values and schooling outcomes. A structural break in that relationship in the year after HEA92 identifies the effect of removing home equity from assets taxable by the financial aid formula. The identifying assumption is that any change in the relationship between schooling decisions and home values after HEA92 is attributable to increased loan eligibility.

The results are mixed. The CPS analysis indicates a positive effect of loan eligibility on college attendance and that loans affect the choice of college, with students shifting toward four-

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[4] One other study uses a similar approach to evaluate the effect of loans. Reyes (1995) analyzes the effect of the passage and repeal of the Middle Income Student Assistance Act of 1978 and concludes that expanding eligibility for loans to middle- and upper-income students had a positive effect on attendance rates.

[5] Since federal grants were level-funded during this period, and very few people with valuable homes are eligible for need-based grants, the bulk of this newly-offered aid took the form of loans.

year private schools. These results suggest that removal of $10,000 in home equity from taxable assets increases college attendance by 0.95 percentage points. This translates into an increase of 1.7 percentage points per $1,000 of loan eligibility. The SIPP provides weak supporting evidence for the CPS results. While the SIPP contains better data than the CPS on assets, the sample is substantially smaller and the estimates quite imprecisely estimated.

Since the paper's estimate are based on variation in eligibility for subsidized loans, we cannot interpret any estimated effect of loan eligibility as a pure liquidity effect. While the offer of a market-rate loan will only affect the behavior of those who cannot otherwise borrow sufficiently at the market rate, i.e., those who are liquidity-constrained, the loans under examination in this paper are not offered at the market rate. The government pays their interest while the student is in school and the interest rate itself is quite low. Such loans are therefore price subsidies which increase the optimal level of schooling for even those who are not credit constrained. Further, HEA92 expanded loan access to a population that was observably unconstrained, in that they hold home equity, an easily-tapped source of liquidity. The price effect of this particular policy shift therefore dominates its liquidity effect.

II. Background

The Stafford Loan, by far the largest student loan program, dates to 1965, when the Guaranteed Student Loan was initiated. From the start, the loans were a joint venture of the public and private sectors. Private lenders provide the loans to students, while the government determines individual eligibility for loans, pays interest on some loans while students are

enrolled in school, and guarantees lenders against default.[6] Interest rates, loan maxima and other loan terms are defined by Congress, generally during the reauthorization of the Higher Education Act of 1965, which occurs every six years.

The loans do not just provide liquidity to students who are temporarily strapped for cash. If enhanced liquidity were the only goal, loans would be offered at a market rate, with interest capitalized into principal while the student was in college. However, the government pays the interest on most Stafford loans while the student is in college. This is a substantial subsidy to college costs. Assume for the moment that loans are offered at the market rate, and so the in-school payment of interest is the only subsidy. If a student borrows $1,000 in his freshman year at a real rate of four percent, spends four years in college, and pays the loan off in ten years, the in-school subsidy saves him $200 over the life of the loan, or 20 percent of its face value.[7] If he discounts the future an annual real rate of four percent, the subsidy is worth 15 percent of the face value of the loan.

A further source of subsidy is the low rate of interest at which Stafford loans are offered to students, which in 2002 is 5.99 percent.[8] In 2002, 5.99 percent may not sound particularly low, since home equity lines of credit are currently available at 4.75 percent. From the government's perspective, however, Stafford loans are uniquely risky. Eligibility is determined by financial

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[6] During the 1990s, the federal government joined banks in providing loans, through the Direct Lending program.

[7] If four more years are spent in graduate school, the government pays interest representing about one third of the face value of a freshman-year loan.

[8] The rate is defined as the 91-day Treasury bill rate plus 2.3 percent and is reset annually on July 1, based on the most recent Treasury auction. Rates paid by students are nominally capped at 8.25 percent, but those paid to lenders are not subject to this cap. Instead, the federal government assumes the risk of rising interest rates above this cutoff.

need, not credit-worthiness; no credit rating at all is required to get a loan. Further, the loans are not secured by any tangible asset. There is no private market for exactly this type of loan, and therefore no observable market rate.

We can get at least a rough sense of the rate on a hypothetical, private-market Stafford loan by looking to similar credit markets. The most closely-related instruments are the credit-rated student loans offered by some private lenders. Rates offered by Sallie Mae, a large player in the loan market, range from 4.75 percent for a borrower with an "excellent" credit rating to nine percent for a borrower with a "fair" credit rating, the lowest rating that allows access to most forms of consumer credit.[9] Loans are not offered to those with poor or no credit histories, and so their rates are unobserved. The Sallie Mae schedule, which averages about seven percent, is therefore a low estimate of the rates that Sallie Mae would offer to a more risky pool of borrowers. This suggests that the Stafford loan, at 5.99 percent, is priced at least a point below the market rate.

For our hypothetical student, borrowing at a real rate of three percent (a point below the posited market rate of four percent), the rate discount brings the subsidy value of the Stafford loan to 30 percent of face value. In other words, if students are rational maximizers, and make the calculations laid out in the preceding paragraphs, then a student offered $1,000 in the form of a Stafford loan should value it about a third as much as $1,000 offered as a tuition discount, and so will respond to loans about a third as strongly as to grants. A substantially lower response than this would suggest that students fail to recognize the subsidy value of the Stafford loan and that, therefore, subsidized loans are a less efficient instrument than grants for subsidizing college.

[9] Advertised rates do not include one-time fees, which are as large as nine percent of principal. I have factored these fees into the rates used in the text.

III. Background

We would like to know how federal loan eligibility influences schooling decisions. Loans may affect the likelihood of going to college, the type of college attended, or years of schooling completed. In an ideal experiment, we would gather a group of high school seniors, randomly offer loans, and over time observe their schooling decisions. In the absence of such an experiment we might use survey data to estimate the relationship between loan eligibility and schooling decisions:

$$(1) \quad y_i = \beta_0 + \beta_1 loan\_eligibility_i + \varepsilon_i$$

In this equation, $loan\_eligibility_i$ is the dollar amount of loans for which person i is eligible.

To fix ideas, consider the case in which y_i is a dummy indicating whether a person has attended college. We would like to interpret β_1 as the causal effect of a dollar of loan eligibility on the probability of college attendance. But we can't reasonably make this interpretation, since loan eligibility is certainly correlated with the excluded determinants of y_i in Equation (1). For example, low-income students are eligible for larger loans, *ceteris paribus*. But low-income youth are relatively unlikely to go to college, for reasons that are vigorously debated: their parents did not go to college and so can provide little information about its costs and benefits; they attend schools of poor quality throughout their lives and are unprepared for college; or they can't afford college even if its cost is subsidized.[10]

[10] For a discussion of the causes of low college attendance among low-income youth, see Cameron and Heckman (1999) and Kane (1999).

One way to deal with this identification problem is to add to Equation (1) a set of covariates correlated with college attendance and loan eligibility:

$$(2)\quad y_i = \beta_0 + \beta_1 loan\_eligibility_i + \beta_2 X_i + \varepsilon_i$$

We can now give β_1 a causal interpretation if we have correctly modeled the direct effect of X_i on college attendance. However, we may not properly model the schooling decision, by either improperly omitting variables from Equation (2) or including them in the wrong functional form. Theory provides us little guidance as to which attributes should be held constant in estimating Equation (2). And even if we correctly model the schooling equation, data on relevant characteristics may simply be unobservable in practice. For example, parental wealth affects schooling decisions, both directly and through eligibility for loans, but complete information on parental wealth is rarely available in survey data.

In sum, the omitted variables problem may be unsolvable using the approach of Equation (2). In order to estimate the causal effect of aid, we need a source of variation in loan eligibility that is plausibly exogenous to the frequently unobserved factors that may influence college attendance. A shift in loan policy that affects some students but not others is potentially such a source of exogenous variation. In the next section, I describe the policy change I use to identify the effect of loan eligibility on schooling outcomes.

The Higher Education Amendments of 1992

Every six years, the Higher Education Act of 1965 expires and must be re-authorized. This act, as currently amended, is the authorizing legislation for the Pell Grant and student loan programs. During re-authorization, Congress generally alters the rules that determine eligibility

for federal student aid. The Higher Education Amendments of 1992 made changes to both loan and grant policy. Its greater impact, by far, was on loan eligibility.

Figure 1 plots loan volume, in real terms, during the 1990s. Federal loan volume was $19 billion during academic year 1992-93, the last before HEA92 took effect. Real loan volume rose dramatically after HEA92, jumping 37 percent in just one year and nearly doubling to $36 billion by 1999-2000.[11] By contrast, federal grant volume was flat over the same period.[12] About half of the growth in loan volume was due to increased borrowing of subsidized loans, which are the subject of the analysis.[13]

To understand the eligibility changes brought about by HEA92, one needs to understand how federal aid eligibility is determined.[14] Months before they enter college, students complete a financial aid application, providing financial information and indicating the colleges they are considering. Their data is run through an algorithm that calculates the amount their families are expected to contribute toward college costs. The algorithm sums income from a variety of sources and subtracts off allowable expenses (taxes, a maintenance allowance based on family size, elementary school tuition and unusually high medical costs). To this figure is added twelve

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[11] See Table 2 in College Board (2000).

[12] In part, this is because grants are limited by appropriations while loans are an entitlement. Marginal changes in aid eligibility will therefore be reflected as increased loans if grants are level-funded.

[13] "Unsubsidized" Stafford loans, which are not distributed on the basis of need, are offered at the same rate of interest as the need-based loans but lack the in-school subsidy. Unsubsidized borrowing accounts for the other half of the increase in loan volume after HEA92. Future versions of this paper will attempt to identify the effect of increased eligibility for unsubsidized loans on schooling decisions. By comparing the effect of unsubsidized and subsidized loans, we can back out the value to students of the in-school subsidy.

[14] This description of aid determination is for the 1991-92 school year and applies to dependent students. In the empirical analysis, I will be focusing on 18-19-year-olds, who are overwhelmingly defined as dependents, in order to avoid the analytical problems caused by students gaming their dependency status.

percent of assets above an "asset protection allowance." For 1991-92, the asset protection allowance ranged from zero to $62,400 and rose with the age of the older parent; at age 45, the allowance was $34,200.[15]

The resulting weighted sum of assets, expenses and income is called "adjusted available income." A progressive tax schedule, with rates ranging from 22 percent to 47 percent, is applied to this figure to determine the family's expected contribution. If the family's expected contribution is less than the student's expected schooling costs, the student is offered a package of grants and loans, with the neediest (as deemed by the aid rules) offered grants and the less needy offered subsidized loans. If the family's expected contribution is more than schooling costs, the student is ineligible for grants or subsidized loans but can obtain unsubsidized loans.

Until HEA92, home equity was one of the assets taxed in this process. As a result, up to 5.64 percent of home equity was considered available for paying a given year's schooling costs. Since the tax is applied each year that a child goes to college, the cumulative tax for a child in college for four years is up to 20.72 percent [$=1-(1-0.0564)^4$]. Should two children attend four years of college consecutively, the tax is as high as 37.15 percent.[16]

A key provision of HEA92 was the removal of home equity from the set of taxable assets. Since home equity is a large proportion of household net worth, this had a major impact on families' paper wealth. Among homeowner households with college-age children in the 1990 Panel of the Survey of Income and Program Participation (SIPP), home equity represents 64

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[15] In determining Pell Grant eligibility, the rule was slightly different: the flat allowance for non-housing assets was $25,000 and an additional $30,000 was subtracted from home equity.

[16] See Feldstein (1995) and Dick and Edlin (1997) for further discussion of this tax and its effect on parental savings.

percent of net worth for the median owner.[17] For a family with the median equity of $45,000, the equity rule change would have reduced the expected family contribution by up to $2,400 for each year that a child was in college.[18] The rule change had its greatest impact on those with the highest home equity: for families with equity at the 75th percentile, the expected family contribution would have dropped by up to $4,800. It is this heterogeneous effect of the rule change on financial aid eligibility that I will exploit in estimating the effect of loans on schooling decisions.[19]

Using Equity to Identify the Effect of Loans

By removing home equity from the financial aid calculation, the government swept many people into the pool eligible for student loans. Since the expected family contribution dropped most for those with the highest home equity, this suggests comparing the college attendance of youth from high and low equity homes, before and after HEA92, and interpreting any increase in the attendance of high-equity youth to HEA92. Alternatively, rather than arbitrarily divide the population into high- and low-equity families, we can instead make the analysis continuous by estimating a time-varying effect of a given dollar of equity on college attendance. The effect of the equity rule change is then identified by any shift in the equity-attendance gradient that occurs after HEA92. In other words, we can estimate the smooth relationship between equity and

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[17] Author's calculations. These figures are based on the 97% of homeowner households that have positive net worth. The median is of the distribution of home equity divided by net worth.

[18] The calculation assumes net worth of $76,700, home equity of $45,000 and an asset protection allowance of $34,200. I assume ineligibility for a Pell Grant, since almost no student with family income at the median is eligible.

[19] Note that the effect of the rule change on loan eligibility is not linear. Some families are so well off that incremental changes in home equity will have no impact on their aid eligibility, and the same is true of families that are quite poor.

attendance and identify the effect of the equity rule change from a structural break in that relationship after HEA92.

We can look for such a shift by estimating the following equation:

$$(3)\quad y_{it} = \alpha Equity_i + \beta Equity_i \times after_t + \delta_t + \mu_{it}$$

Here, y_{it} is a measure of schooling, $Equity_i$ is a measure of the home equity held by the family of person i, $after_t$ is a dummy that takes value one as of 1993, when the equity-rule change took effect, and δ_t is a full set of year dummies. A non-zero coefficient on β indicates that the effect of equity on schooling decisions has shifted over time. In order to determine whether this shift occurred around the time of HEA92, I will also estimate a specification in which the effect of equity is allowed to vary by year:

$$(4)\quad y_{it} = \underbrace{\sum_t \beta_t Equity_i}_{home\ equity \times year\ effects} + \delta_t + \mu_{it}$$

One problem with this approach is that for families of college-bound children home equity may be endogenously determined by the financial aid rules. Indeed, there is a small literature that demonstrates that the financial aid rules affect both the level and composition of assets held by families.[20] In the case at hand, HEA92 may have induced families of college-bound youth to shift their savings into home equity in order to protect them from taxation. This would produce a spurious shift in the relationship between equity and college attendance after HEA92. In order to

[20] Feldstein (1995) and Dick and Edlin (1997) calculate the magnitude of the asset tax and estimate its impact on asset accumulation. Kim (1997) shows that families move their savings into protected assets (e.g., retirement vehicles) in order to avoid having them taxed by the financial aid formula.

avoid this problem, and because of data constraints described in the next section, I will in most specifications proxy or instrument for home equity with home value or local real estate values.

IV. Data

I use two household surveys, the October School Enrollment Supplement of the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP), to estimate the equations described above.

CPS

The strength of the CPS is its size and frequency. Because it is an annual survey, we can use it to distinguish sharp breaks from continuous trends, and the large sample allows us to do so with quite small correlations. The drawback of the CPS is that it does not allow us to link all college-age children with their parents; only 74 percent of 18- and 19-year-olds have parental information available. Rather than risk biasing the sample by throwing out the quarter of youth who do not have parental information, I restrict the CPS analysis to specifications that do not require parental data.[21]

A consequence is that in the CPS we cannot use income data to narrow in on those youth whose financial situation places them on the margin of receiving more aid. This is a serious drawback, since the effect of equity on aid eligibility is non-linear. Home equity has no impact on the loan eligibility of the poorest and richest families. They are either so poor that they are already eligible for the maximum allotment, or so rich that they are ineligible even with the

[21] A college-age youth can be linked to her parent's CPS record if she lives with her family or she is away at college, but not if she is living on her own and not attending college. This will produce bias in analyses where college attendance is an outcome of interest and the sample is limited to those with parental information. Cameron and Heckman (1999) discuss this point.

exclusion of home equity from their assets. By necessity, in the CPS I can identify only the average effect of the equity rule change, calculated over those who are and not on the margin of getting more aid.

The CPS does not contain information about a household's home equity. I therefore use state-year median home values to proxy for individual home values. I calculate these home values as follows. The Federal Home Loan Mortgage Corporation (known as Freddie Mac) maintains a quarterly, state-level index that tracks repeat sales of single-family homes.[22] The index is unitless; for the analysis it has been normed to one in the first quarter of 1990. To anchor the index in dollar terms, I calculate median home values in each state using the 1990 census and multiply these values by the index. The home values are then converted to 2000 dollars using the CPI price index for shelter. This gives me a state-by-year panel of home values.[23]

Much of the variation in home values is due to fixed differences across states: California is always more expensive than Arkansas. State fixed effects will be included in the regressions to absorb these fixed differences. But there is considerable within-state variation in home values. Figure 2A graphs median state home values in 1984, sorted from lowest to highest. Median home values varied from $78,233 in Mississippi to $383,201 in Hawaii. Figure 2B shows home values in 1999, with the states ordered by their 1984 ranking. As is clear from Figure 2B, there is considerable movement in relative home values during this period.

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[22] By focusing on this type of sale, the index excludes price variation that is driven by changes over time in housing quality and size.

[23] The index is available by metropolitan area. Changing borders of MSAs over time make it difficult to construct a consistent panel. When state-year median home value is included in the regressions, standard errors are adjusted for correlation within state-year cells.

The CPS sample spans 1984 to 2000 and consists of all 18-19-year-olds. I measure family income, home value and home equity in this January survey. The outcomes of interest are college attendance and the type of college attended. In the CPS, I observe whether a person is enrolled in October and the type of college he attends.

Means of the CPS data are in Table 1. Sample weights are used throughout the analysis.

SIPP

The SIPP is a longitudinal survey, with a new panel started periodically. I use the panels that began in 1986, 1987, 1990, 1992 and 1996. Because SIPP is a longitudinal survey, we can identify family groups when children are of high school age and observe later whether those children go to college. The SIPP contains data on households' home equity and home value in the Assets and Liabilities topical module. The questions in this module are asked of a household in the first months of a given calendar year, which is about the time that a child would be filling out financial aid forms for college. Two waves later, I can observe fall college enrollment.

Ideally, the sample would consist of those who were high school seniors when the assets questions were asked or recent high school graduates the following fall. Unfortunately, the wording of the completed schooling variable changed as of the 1996 panel of the SIPP, and I am unable to construct a consistent sample of such youth across the years. Instead, I base the sample on school enrollment status. I include in the sample those who were enrolled in high school during a given winter (when the asset questions were asked) but were no longer enrolled in high school as of the following fall, when they were 18 to 19 years old. Of this sample, 93 percent appeared on their parent's record when the asset questions were asked and 58 percent were enrolled in college as of the fall.

Means of the SIPP data are in Table 2. Sample weights are used throughout the analysis. Note that each youth appears in these data only once, as I am using the panel aspect of the SIPP only to match college-age youth to their parents' households and not to follow the youth themselves over time. Due to the timing of the asset questions, I have data on the fall enrollment of youth in the falls of 1987, 1988, 1991, 1993 and 1998.

V. Results

In the first column of Table 3 are the results of estimating Equation (3). During the years preceding HEA92, each $10,000 of home value was associated with an increase in the state's college attendance rate of 0.106 percentage points. During the years after HEA92, this relationship was significantly more positive. The interaction term, which is highly significant, indicates that after HEA92 each $10,000 increase in home value was associated with a rise in the attendance rate of 0.348 percentage points. The interaction term is of the expected sign: each dollar of home equity is more valuable after HEA92 than before, since it is no longer taxed by the financial aid formula.

In the next column, I add state fixed effects to the regression. The main effect of home value is now negative, indicating that a state's average level of college attendance, as captured by the fixed effects, is strongly and positively correlated with its average home values. The addition of the state effects does not substantially affect the interaction term, however, which is still positive (0.165) and more precisely estimated (t=3.5).

In Column (3), I add to this regression an extensive set of individual and state-level covariates. Since rising home values in a state may reflect a tight labor market, which will increase the opportunity cost of college, I include the state's September unemployment rate, as

well as the change in the unemployment rate over the previous year.[24] Household income is likely correlated with both home values and college attendance, and I therefore include the mean and median income of households with children who are nearing college age.[25] Individual covariates consist of dummies for an individual's age, race and ethnicity. The coefficient rises somewhat, and is quite close to that obtained from the leaner specification of Column (1). Last, in order to capture any change in the effect of covariates over time, I interact all of the covariates with a dummy for the post-HEA92 years. To absorb any regional trends in attendance rates that are correlated with home values, I also include a set of nine region dummies interacted with the after-HEA92 dummy. While there is some loss of precision, the interaction term is still positive (0.397) and significant (t=3.0).

The results of Table 3 indicate that home equity was more positively correlated with college attendance after HEA92 than before. This may be driven by a positive trend in the effect of home equity that precedes HEA92. In order to inspect the evolution of the coefficient over time, I loosen the specification of Column (4) by allowing the effect of home equity to vary by year. The coefficients on these interactions are shown in Table 4 and plotted in Figure 3. In the figure, the 1992 interaction is normed to zero. The effect of home values on attendance is becoming more positive throughout this period, rising from –1.01 percentage points in 1984 to –

[24] September unemployment plausibly captures the conditions facing a youth deciding whether to enter college in the fall. Using annual unemployment does not change the results.

[25] These younger children are less likely than 18-19-year-olds to have split off and formed their own households, and so this measure more reliably captures parental income, which determines aid eligibility. The income variable is categorical and top-coded. I assign the midpoint of each category except for top-coded households, which are assigned the highest coded value for that year. Adding to the regressions a variable measuring the share of the state's households that are top-coded in a given year does not affect the results.

0.73 percentage points in 1992. In 1993, however, there is a sharp break in this series, with a one-year rise of 0.27 percentage points. This is the largest change in the series, though the graph is obviously quite noisy, particularly after 1992. Further evidence of a break in the series is that while the coefficients rise quite smoothly before 1992, they follow no discernable trend subsequent to the 1993 increase.

In order to compactly parameterize Figure 3, I fit a new model to the data. I estimate separate time trends in the effect of home values before and after HEA92, as well as an intercept shift in 1993:

$$(5)\quad y_{ist} = \alpha_0 + \alpha_1 after_t + \alpha_2 equity_{st} + \alpha_3 time_t \times equity_{st} + \alpha_4 time_t \times equity_{st} \times after_t + \alpha_5 equity_{st} \times after_t + \alpha_6 X_i + \mu_{ist}$$

This specification includes all of the covariates included in the previous specifications. Results are in the first column of Table 5. Before HEA92, the effect of home values on attendance grows more positive at the rate of 0.04 percentage points per year (with a standard error of 0.02), growing more slowly after 1993 at a rate of 0.016 percentage points per year.. In 1993, there is an (imprecisely estimated) increase of 0.22 percentage points in the effect of $10,000 of home value on the attendance rate. Based on the pre-existing time trend, the increase in the effect in 1993 was five times larger than expected.

These results suggest that the removal of equity from the assets taxable by financial aid had a positive, albeit imprecisely estimated, effect on attendance rates. It is possible that the policy change also had an impact on other aspects of schooling decisions, such as the type of school attended. In order to address this question, I take the specification of Column (1) in Table

5 and run three regressions in which the dependent variables are dummies for attendance at three types of colleges: two-year public, four-year private and two-year public.[26]

As is clear from Table 4, the effect is concentrated in four-year private colleges. After HEA92, there is a significant rise in the effect of $10,000 in equity on attendance at these schools of 0.2 percentage points (with a standard error of 0.09 percentage points). By comparison, the effects for attendance at four-year public and two-year public colleges are small and insignificant. A plausible explanation for this set of coefficients is that increased access to loans induced some youth who would not have attended college at all into public colleges and others who would otherwise have attended public colleges into private schools.

SIPP: Focusing on the Affected Population

The CPS results suggest a structural break in the relationship between home values and schooling decisions after HEA92. With this evidence in hand, I turn to the SIPP in order to confirm that the effect is concentrated among those youth whose eligibility for loans was most likely increased by HEA92.

Table 6 replicates the attendance analysis of the previous section using the SIPP, with college attendance as the outcome. The covariates used in the previous section are used, except income and asset data are now measured at the household level. The sample is limited to homeowners, since own home value and equity are now the explanatory variables of interest. This ability to focus on homeowners is an advantage of the SIPP, since, in the CPS, we could not identify which college-age youth were from homeowner families. In Column (1), I estimate the

[26] Only one percent of this age group attends a two-year private school, so I do not include these results. For this type of college, the coefficient of interest is small, negative and statistically insignificant.

change in the effect of home equity on attendance after HEA92, using the specification of Equation (3). The estimate is positive and significant: 0.691 percentage points, with a standard error of 0.335 percentage points. The main effect indicates that the probability of college attendance rises about a percentage point with each $10,000 increase in home equity. Note that while here the equity main effect is positive, in the CPS the median home value main effect was negative in most specifications. This suggests that the coefficient on median home value was picking up the effect of unobserved local economic conditions that increased the opportunity cost of schooling. Using equity rather than home value, and measuring the variable at the household rather than the state level, appears to eliminate this source of bias, yielding a consistently positive relationship between housing wealth and college attendance rates.

Adding covariates decreases the main effect of equity and its interaction with "after" somewhat, and increases the standard errors. The specification that includes state effects, year effects, individual characteristics and the state unemployment rate yields a coefficient on the interaction term of 0.539 and on the main effect of 0.396 (with standard errors of 0.347 and 0.258, respectively).

In response to HEA92, homeowners with college-bound children may have shifted assets into their now-protected home equity. If parents of children who plan to go to college engage in this sort of strategic reallocation after HEA92, then we will observe a spuriously high coefficient on the interaction term. Alternatively, home equity may be measured with error, and the coefficient on both equity and its main effect will then be too close to zero.

In order to deal with these sources of bias, I use an instrumental variables strategy. As instruments for home equity and its interaction with the after dummy, I use home value and its interaction with the after dummy. Results are in Table 7. The main effect of home equity rises

substantially. In the most fully-controlled specification of Column (3), the IV equation yields a coefficient of 0.749 on home equity, with a standard error of 0.304, vs. the OLS estimate of 0.396 in Table 6. That this IV estimate is substantially larger in magnitude than OLS suggests that home equity is, indeed, measured with error. By contrast, the coefficient on the interaction term drops in magnitude when we instrument for equity, indicating that strategic asset allocation was inflating the OLS estimate.

Taking the specification of Column (3), the OLS estimates indicated that the attendance rate rose by 0.396 percentage points for each $10,000 of home equity pre-HEA92 and by 1.135 percentage points after HEA92. The IV estimates indicate that attendance rate rose by 0.749 percentage points for each $10,000 of home equity pre-HEA92 and by 1.013 percentage points after HEA92. In other words, the post-HEA92 estimate is just about the same in IV and OLS, indicating that measurement error and bias induced by reallocation of assets roughly cancelled each other in this period.

I next look at whether the effect varies by income. I split the SIPP sample into three household income groups: less than $40,000, $40,000 to $80,000 and over $80,000. I then allow the effect of equity to vary by income. Dummies for the income groups, and the interaction of these dummies with "after" are also included. Results are in Table 8 for both OLS and IV specifications. In every specification, the interaction of equity and after is most positive for the lowest income group. This is the group most likely to get federal financial aid; the Pell Grant is given to very few families with incomes above $40,000. Since subsidized student loans are less concentrated, we would expect to see some effect in the middle-income group, as well. However, this is not the case. The estimates consistently indicate that the effect of equity on attendance

decreased for middle- and high-income families after HEA92. This suggests that the identification strategy is not effectively isolating the effect of HEA92.

Finally, I examine the time pattern of the estimates. In Table 9, I allow the effect of home equity to vary by year. I include in the specification all of the individual covariates (except the income dummies) and state unemployment, as well as their interaction with the year dummies. These results show no clear pattern in the evolution of the effect of home equity. The OLS results do show that the effect became sharply more positive between 1991 and 1993, but that this increase had dissipated by 1998. The IV results do not show even this pattern, with a sharp increase in the coefficient occurring in 1991, before HEA92. All of the estimates are quite imprecise.

VI. Discussion and Conclusion

The CPS results suggest that removing $10,000 in home equity from assets taxable by the aid formula increases college attendance by about 0.95 percentage points. In order to scale this effect, note that each $10,000 removed from assets translates into an increase in aid of up to $564.[27] The effect of 0.95 therefore translates into 1.7 percentage points per $1,000 of loan eligibility. Given the calculations in Section II that the subsidy value of a loan is about a third of its face value, this suggests that $1,000 in loan subsidy induces an increase in college attendance of 5.1 percentage points (with a standard error of 2.9 percentage points). This is roughly comparable to estimates of the effect of grant aid or tuition prices on college attendance, which

[27]Families with very low or high expected family contributions will see little change in their aid eligibility.

are three to five percentage points per $1,000.[28] That is, the point estimate indicates that a dollar delivered as a loan subsidy and a dollar delivered as a grant have roughly the same effect on the probability of college attendance. However, the standard error of the estimate does not rule out that the effect of a loan is substantially higher or lower than that of a grant.

The SIPP provides weak supporting evidence for the CPS results. While the SIPP contains better data than the CPS on assets, the sample is substantially smaller and the estimates quite imprecisely estimated. The simplest OLS specifications (Table 6) produce coefficients consistent with the CPS. However, attempts to differentiate the effect by income, in order to determine whether the effect falls among those on the margin of loan eligibility, yield inconclusive results (Table 8). Further, attempts to pinpoint whether the difference-in-difference estimates reflect a sharp change after HEA92, as the identification strategy requires, are also unsuccessful in the SIPP (Table 9). IV results in the SIPP indicate that measurement error in home equity may be biasing some of the estimates toward zero (Table 7) but, again, the results are quite imprecise.

It should be emphasized that the paper's estimates are for the marginal eligible youth. Those who gained eligibility due to the removal of equity from assets taxable by the financial aid formula are from higher-income households than those who were already eligible for loans. It is ambiguous whether the estimates provide a lower or upper bound for the effect of loans on lower-income youth, because while they are more likely to be liquidity constrained they may also be more debt averse.

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[28] See Dynarski (2000, 2001) and Kane (1994).

Since this estimate is based on variation in eligibility for subsidized loans, we cannot interpret this measured effect of loan eligibility as a pure liquidity effect. While the offer of a market-rate loan will only affect the behavior of those who cannot otherwise borrow sufficiently at the market rate, i.e., those who are liquidity-constrained, the loans under examination in this paper are not offered at the market rate. The government pays their interest while the student is in school and the interest rate itself is quite low. Such loans are therefore price subsidies which increase the optimal level of schooling for even those who are not credit constrained. Further, HEA92 expanded loan access to a population that was observably unconstrained, in that they hold home equity, an easily-tapped source of liquidity. The price effect of this particular policy shift therefore dominates its liquidity effect.

Table 1:
CPS Means
October, 1984-2000

College Attendance in October	0.4311
Two-Year Public College	0.1339
Four-Year Public College	0.2048
Four-Year Private College	0.0791
State Median Home Value/$1M	0.1508 (0.087)
Black	0.1510
Hispanic	0.1189
Age	18.50 (0.50)
State Median Income/$10K	4.206 (0.91)
State Mean Income/$10K	4.519 (0.63)
September Unemployment Rate	0.056 (0.017)
N	63,717

Notes:
Dollar amounts are in constant 2000 dollars. Income is the median or mean in a given state and year of income for households with at least one individual age 16 to17.

Table 2:
SIPP Means
1990 and 1992 Panels

	All	Owners	Renters
College Attendance	0.4211	0.4609	0.3209
Own Home Equity/$10K		8.363 (9.094)	
Own Home Value/$10K		13.90 (10.05)	
Black	0.1433	0.1065	0.2362
Hispanic	0.1120	0.0757	0.2033
Age	18.32 (0.2825)	18.35 (0.2882)	18.31 (0.2798)
Household Income/$10K	5.721 (4.147)	6.621 (4.263)	3.453 (2.750)
Home Owner	0.7160		
N	1284	932	352

Notes:
Dollar amounts are in constant 2000 dollars. Income is measured in the January preceding the academic year in which college attendance is measured.

Table 3:
October CPS, 1984-2000
Home Values and College Attendance
18-19-year-olds
OLS Regressions

	(1)	(2)	(3)	(4)
Home Value*After	0.242 (0.086)	0.165 (0.047)	0.258 (0.061)	0.397 (0.130)
Home Value/$10K	0.106 (0.061)	-0.553 (0.00)	-0.572 (0.111)	-0.575 (0.116)
Year Effects	Y	Y	Y	Y
State Fixed Effects		Y	Y	Y
Individual Covariates: Race/Age/Ethnicity Dummies			Y	Y
State-Year Covariates: Unemployment Rate, Mean & Median of Income			Y	Y
Covariates interacted with After, plus Region*After				Y
R^2	0.003	0.013	0.052	0.052
N	63,717	63,717	63,717	63,717

Note:
Regressions are weighted by CPS sample weights. Standard errors are adjusted for heteroskedasticity and correlation within state-year cells. "Income" is state-year mean and median of household income of 16-17-year-olds. Dollar amounts are inflated to 2000 values. The coefficients on the home value variables have been multiplied by 100.

Table 4: Interaction Terms, by Year

Home Value*2000	-0.289 (0.214)
Home Value*1999	-0.529 (0.204)
Home Value*1998	-0.455 (0.217)
Home Value*1997	-0.268 (0.254)
Home Value*1996	-0.484 (0.205)
Home Value*1995	-0.497 (0.214)
Home Value*1994	-0.368 (0.208)
Home Value*1993	-0.503 (0.179)
Home Value*1992	-0.730 (0.138)
Home Value*1991	-0.784 (0.133)
Home Value*1990	-0.693 (0.143)
Home Value*1989	-0.795 (0.160)
Home Value*1988	-0.883 (0.174)
Home Value*1987	-0.889 (0.199)
Home Value*1986	-0.928 (0.236)
Home Value*1985	-1.105 (0.225)
Home Value*1984	-1.010 (0.246)

Notes: Regressions are weighted by CPS sample weights. The covariates, including fixed effects and interactions, are those included in the specification of Column (4) in Table 3. Standard errors are adjusted for heteroskedasticity and correlation within state-year cells. The coefficients on the home value variables have been multiplied by 100.

Table 5:
College Attendance and College Choice
CPS 1984-2000

	(1) College Attendance	(2) Two-Year Public	(3) Four-Year Public	(4) Four-Year Private
Home Value* After	0.223	0.047	0.045	0.201
	(0.147)	(0.100)	(0.119)	(0.093)
Home Value/$10K	-0.643	-0.254	-0.273	-0.005
	(0.133)	(0.094)	(0.114)	(0.072)
Home Value*Time* After	-0.021	0.022	-0.061	0.019
	(0.030)	(0.019)	(0.028)	(0.019)
Home Value*Time	0.037	0.003	0.049	-0.023
	(0.018)	(0.014)	(0.018)	(0.014)

Notes: Regressions are weighted by CPS sample weights. The covariates, including fixed effects and interactions, are those included in the specification of Column (4) in Table 3. Standard errors are adjusted for heteroskedasticity and correlation within state-year cells. The coefficients on the home value variables have been multiplied by 100.

Table 6:
SIPP
Home Values and College Attendance
18-19-year-old recent high school students
OLS Regressions

	(1)	(2)	(3)	(4)
Home Equity*After	0.691 (0.335)	0.634 (0.347)	0.539 (0.347)	0.552 (0.386)
Home Equity/$10K	0.969 (0.230)	0.808 (0.254)	0.396 (0.258)	0.393 (0.272)
Year Effects	Y	Y	Y	Y
State Fixed Effects		Y	Y	Y
Individual Covariates: Race/Age/Ethnicity Dummies			Y	Y
State-Year Covariates: Unemployment Rate			Y	Y
Time-Varying Covariates: Individual and State covariates interacted with After. Region*After also included.				Y
R^2	0.037	0.078	0.125	0.131
N	1,398	1,398	1,398	1,398

Notes:
Data are for SIPP panels 1986, 1987, 1990, 1992 and 1996. Attendance is a dummy for having attended college anytime in the fall. Income is parental income and is measured the year before attendance. Standard errors are adjusted for heteroskedasticity. The coefficients on the home equity variables have been multiplied by 100.

Table 7:
SIPP
Home Values and College Attendance
18-19-year-old recent high school students
IV Regressions

	(1)	(2)	(3)
Home Equity*After	0.507	0.432	0.264
	(0.449)	(0.463)	(0.454)
Home Equity/$10K	1.484	1.451	0.749
	(0.264)	(0.299)	(0.304)
Year Effects	Y	Y	Y
State Fixed Effects		Y	Y
Individual & State-Year Covariates			Y
N	1,398	1,398	1,398

Notes:
Home equity and home equity*after are instrumented using home value and home value*after. Data are for SIPP panels 1986, 1987, 1990, 1992 and 1996. Attendance is a dummy for having attended college anytime in the fall. Income is parental income and is measured the year before attendance. Standard errors are adjusted for heteroskedasticity. The coefficients on the home equity variables have been multiplied by 100.

Table 8:
SIPP
Home Values and College Attendance, by Income
18-19-year-old recent high school students
OLS & IV Regressions

	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV	(6) IV
High Inc* Home Equity*After	-1.024	-0.886	-0.961	-2.391	-2.202	-2.235
	(1.009)	(1.028)	(0.994)	(1.507)	(1.552)	(1.493)
Mid Inc* Home Equity*After	-1.289	-1.182	-1.032	-2.628	-2.404	-2.194
	(1.059)	(1.062)	(1.023)	(1.549)	(1.587)	(1.523)
Home Equity*After	1.539	1.406	1.287	2.214	2.013	1.867
	(0.880)	(0.898)	(0.856)	(1.336)	(1.383)	(1.313)
High*Home Equity	-0.306	-0.461	-0.473	-0.117	-0.189	-0.382
	(0.712)	(0.713)	(0.696)	(0.798)	(0.815)	(0.797)
Mid*Home Equity	0.664	0.433	0.426	0.488	0.287	0.299
	(0.700)	(0.695)	(0.671)	(0.778)	(0.792)	(0.764)
Home Equity	0.547	0.598	0.460	0.959	1.064	0.862
	(0.609)	(0.613)	(0.599)	(0.664)	(0.686)	(0.672)
High Income, Mid Income, High*After, Mid*After	Y	Y	Y	Y	Y	Y
Year Effects	Y	Y	Y	Y	Y	Y
State Effects		Y	Y	Y	Y	Y
Individual & State-Year Covariates			Y	Y	Y	Y
N	1,398	1,398	1,398	1,398	1,398	1,398

Notes:
In IV columns, home equity and its interactions instrumented with home value and its interactions. The coefficients on the home equity variables have been multiplied by 100.

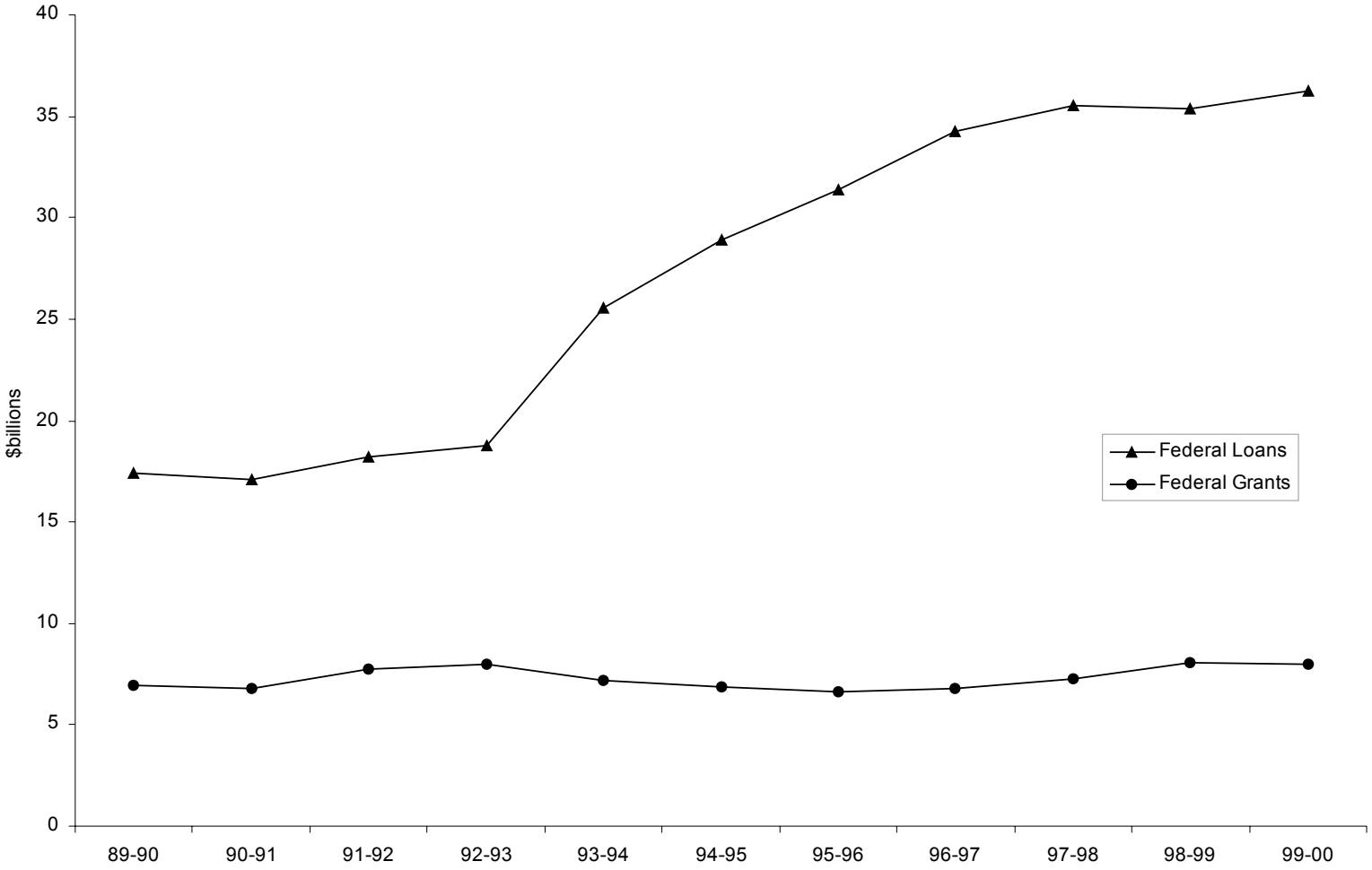
Table 9: Interaction Terms, by Year
SIPP

	(1) OLS	(2) IV
Home Equity*1998	0.380	0.570
	(0.737)	(0.972)
Home Equity*1993	1.090	0.828
	(0.703)	(0.892)
Home Equity*1991	0.367	1.225
	(0.746)	(0.896)
Home Equity*1988	0.595	0.666
	(0.693)	(0.810)
Home Equity*1987	0.093	0.135
	(0.610)	(0.722)

Notes:
In IV columns, home equity and its interactions instrumented with home value and its interactions. The coefficients on the home equity variables have been multiplied by 100. Covariates of previous tables included, plus (with the exception of state dummies) their interaction with year dummies.

Figure 1: Loan and Grant Volume

Loan provisions of HEA92 first effective AY93-94



Note: Figures are from College Board (2000). Values are inflated by the CPI-U with academic year 1999-2000 as the base year.

Figure 2A: Median Home Values in 1984

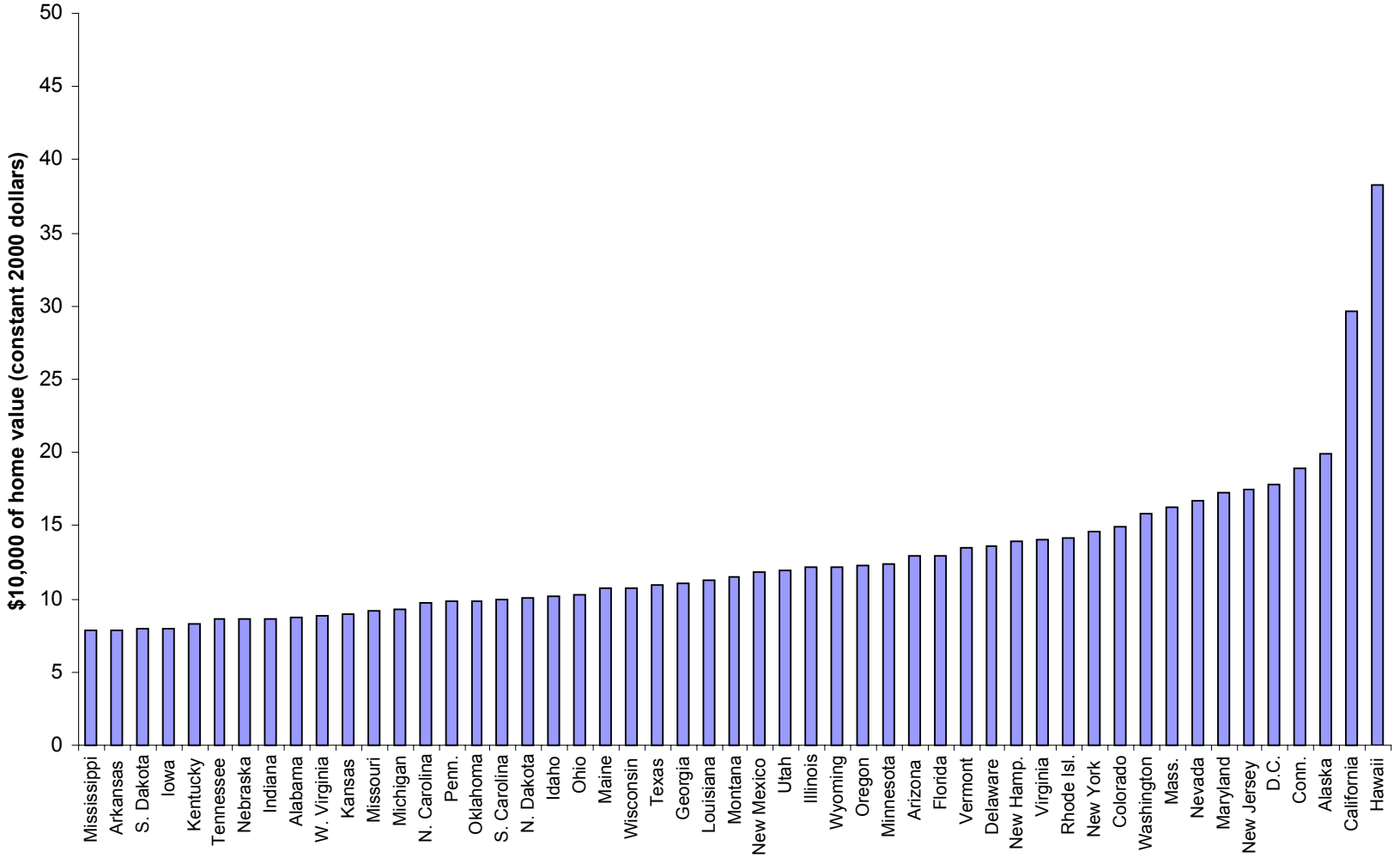


Figure 2B: Median Home Values in 1999

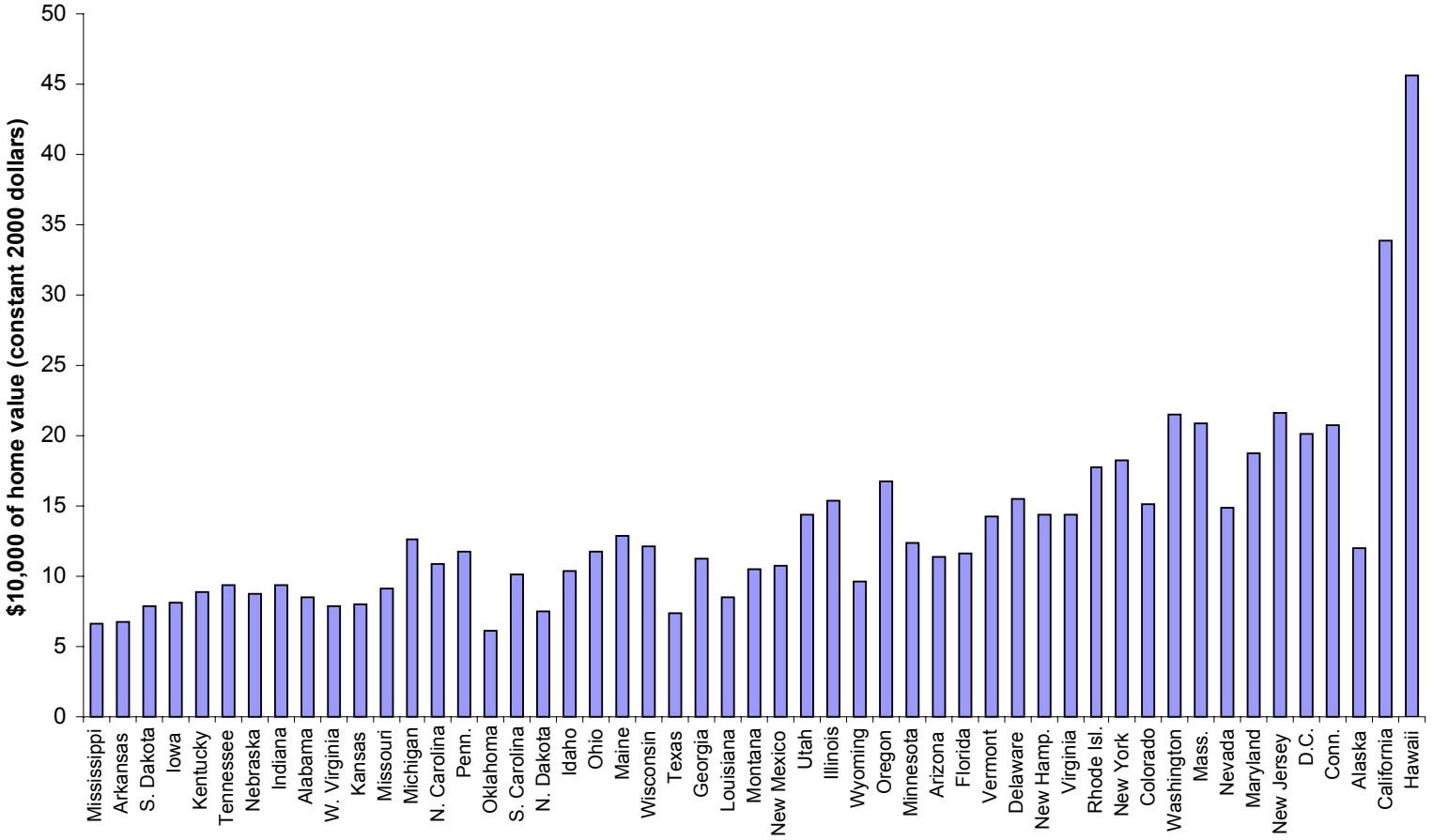
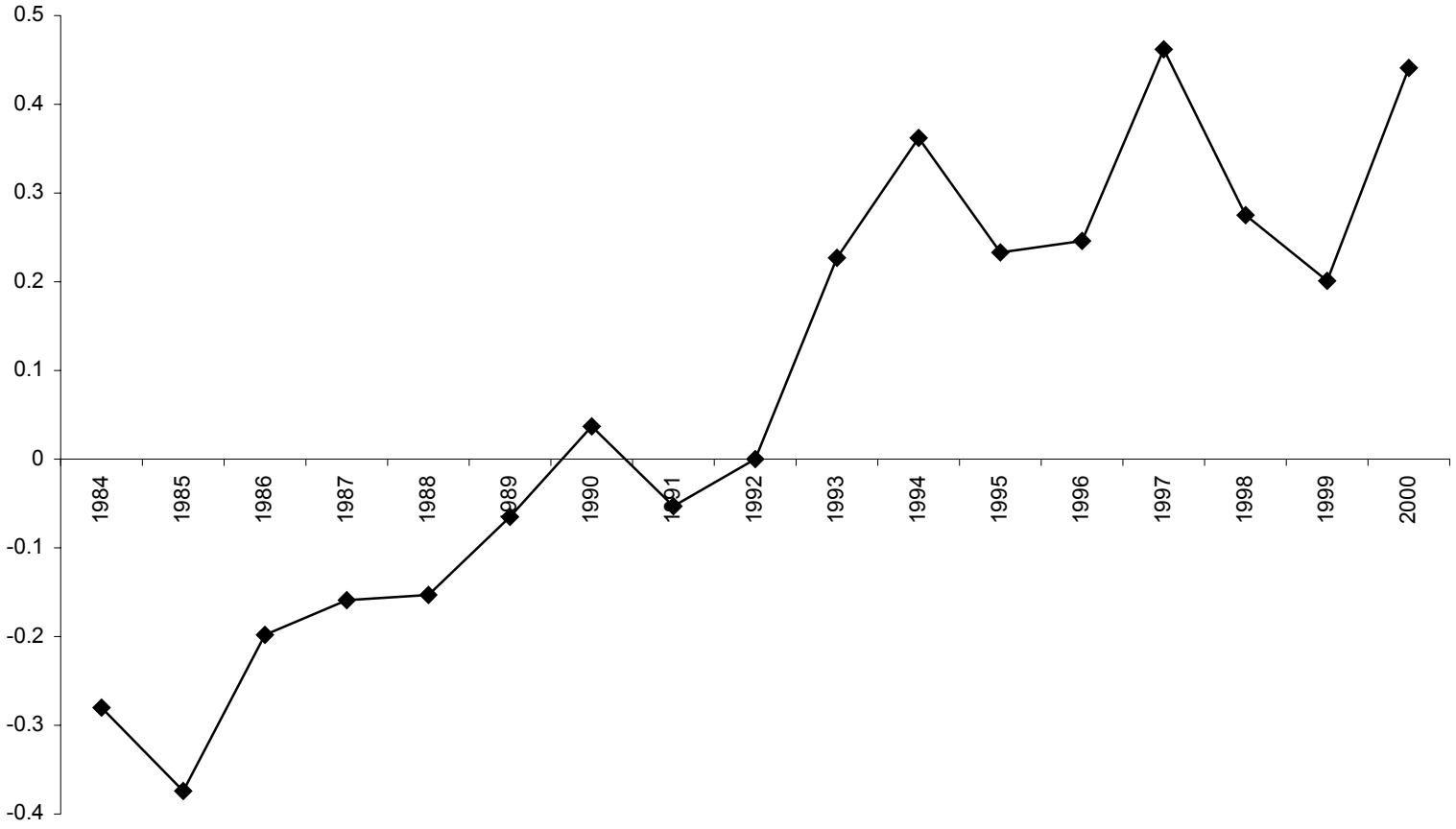


Figure 3: Effect of Home Value on Attendance, by Year

1992 effect normed to zero



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